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The Role of Science In the Public Arena

Arthur B. Weissman
AGU Congressional Science Fellow

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On the other hand, the political complaint about 'two-handed' scientists suggests with some justification that scientists are often not 'up front' about the meaning and significance of their work. It is not just that scientists play an ambiguity and often refuse to take sides in controversial issues; politicians of course do this too (and more capably). But because they are not often required to justify their work, as politicians are, scientists are not always as conscientious in putting their work together and explaining its purposes clearly in their writings.

My sudden transition from academic science to the political arena highlighted for me the differences between the two, which are striking and at times disconcerting. Academic scientists, laden with teaching and often administrative responsibilities, are fond of complaining of the lack of time for creative research and thinking. But, for the most part, they are in an environment which supports and encourages research and independent thinking. In the political world such words as 'professor' and 'research' can be pejorative or, at best, quaint; professors and research re-

searches may be taken seriously only when they support a given political viewpoint.

On the other hand, the political complaint about 'two-handed' scientists suggests with some justification that scientists are often not 'up front' about the meaning and significance of their work. It is not just that scientists play an ambiguity and often refuse to take sides in controversial issues; politicians of course do this too (and more capably). But because they are not often required to justify their work, as politicians are, scientists are not always as conscientious in putting their work together and explaining its purposes clearly in their writings.

Despite these obvious differences, there is a parallel between scientific and political positions in the way factual information is used, and this can be useful in understanding the role of science in public policy. A scientific position (conclusion, finding, or theory) is developed from a set of facts about natural phenomena; but, particularly when a position is controversial or preliminary, the facts used to support it are selected among a much larger array of facts and related phenomena, not all of which necessarily support that position. (Of course, a reasonable position must not be strongly contradicted.) In a similar way, political positions are developed, justified, and defended on the basis of a certain set of facts. Those who hold opposite positions appeal to another set of facts, often complementary to the first. It is the selection and combination of facts that, as in a scientific position, make the case for a political viewpoint.

The challenge for a scientist in the public arena is to be faithful to both ends of the decision process, scientific and political. One must first get an accurate scientific picture of a problem, using the best scientific judgment in the selection and consideration of facts. Then one must put this scientific picture in the political context and determine what political, economic, and social forces impinge on it. Parts of the picture may emerge more clearly or forcefully and parts may fade from view, but the original scientific picture should not thereby become distorted. Policy will emerge from all of these considerations, and if the politics on which it is based are reasonably wholesome, none of the 'facts' on which it is based should be compromised.

This combination of science and policy is well illustrated in the current issue of acid precipitation. One can easily get immersed in the complexities and uncertainties of the scientific issue in considering reaction rates, catalysis and liming factors, transport from source to receptor, and effects on soils, lakes, and biota. Although many of the critical links in acid precipitation are not yet scientifically established, the overall picture—the trend—is apparent to some extent, and both the public and its public servants are demanding 'facts' from scientists to justify either a program of control or a policy of delay. It is clear, however, that this scientific certainty can not determine a decision on acid precipitation control, for that is ultimately a political choice among values: ecological and possible health

effects compete with economic costs and social disruption. The scientific aspect of the issue concerns what, where, and how much to control; but the decision of whether or not to control acid precipitation and to what degree is ultimately political.

Thus, a Congressional Fellow must extend the comfortable universe of scientific knowledge into a somewhat alien realm where it must compete with other considerations on a more or less equal basis. Congress is, after all, like a vast marketplace, and politics at its best is the making of choices among the 'common wealth.' Certainly only by understanding, and not ignoring, the political process can scientists hope to see public policies made in accordance with good scientific thinking.

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Arthur B. Weissman is AGU's 1983 Congressional Fellow. He received his Ph.D. last year from the Johns Hopkins University Department of Geography and Environmental Engineering for his dissertation on the aesthetics and geomorphology of environmental change. He received his A.B. from Harvard College in 1970 and an M.F.S. degree in natural resource management from Yale University's School of Forestry and Environmental Studies in 1975. From 1975 to 1977 he was Assistant Director of the Nature Conservancy's Connecticut chapter. He has conducted research at the U.S. Geological Survey, the National Oceanic and Atmospheric Administration, and the U.S. Forest Service, and has held teaching positions at the Léman American School in Lausanne, Switzerland, at Maryland's Towson State University, and at Johns Hopkins University.

News

Mn Solubility Tested in Seawater

It has been known for the past 2 or 3 years that the concentration of manganese in the upper several hundred meters of ocean water is unlike that of other trace metals such as copper, zinc, cadmium, and nickel. Trace metals are needed as a sort of 'vitamin supplement' by marine plants and animals; the surface supply is biologically scavenged and regenerated at depth. Thus ocean concentrations of trace metals increase with depth.

Manganese, by contrast, appears to be concentrated in the photic zone and becomes relatively depleted in the depth interval 50-100 m from the surface. W. Landing and K. Bruland (*Ear. Plan. Lett.* 49, 45-56, 1980) described their observations of vertical distributions of manganese in a study of samples from the north Pacific. Recently, W. Sunda, S. A. Hunziker, and A. Harvey, in a study supported by the National Oceanic and Atmospheric Administration have found similar behavior of manganese in samples of coastal seawater and offshore seawater collected off North Carolina (*Nature*, 20, January 1983).

They suggest on the basis of experiments conducted with these samples that the marine biological community itself serves to condition the surface seawater and, with the assistance of photoreduction, cause manganese to dissolve in the otherwise oxygenated zones. The process in the oceans would be similar. At the ocean surface there is an abundance of sunlight and of organic matter that itself has developed under the influence of photosynthetic processes. The conditions appear to be favorable for the photoactivated reduction of manganese by dissolved organic matter. Manganese in this state would be dissolved, explaining observed marine profiles. The amount of sunlight and the content of dissolved organic matter decreases with depth in the ocean, so this would explain the decrease in manganese concentration. It is observed that there is an increase in particulate MnO₂ concentration near the bottom of the photic zone. Such particles would sink eventually.

Landing and Bruland arrived at much the same conclusion about the source of the manganese to their analyses of surface water manganese, they also measured the ²¹⁰Pb concentrations. They found, however, that the Mn cycle of deposition to the ocean surface and later removal from the water by scavenging is not generally coupled with the ²¹⁰Pb cycle, whose surface distribution is characterized by atmospheric input. They concluded that unlike ²¹⁰Pb, most surface manganese is derived from rivers, estuaries, and near-surface shelf sediments. They note as well that manganese concentrations follow reduction zones in the oceans, corresponding to the oxygen minimum zones.

At the surface, though, oxygen is not at a minimum, and Sunda et al. think that the process they postulate not only is responsible for the geochemical concentrations of manganese at the surface but, in circular, is an important biological process. Deep seawater that is upwelled contains insufficient manganese to support maximum plant growth; they note

the Gulf Stream. They also extracted some marine humic acid from seawater collected at a depth of 5 m in a station located 110 km south of Cape San Blas, Florida. The idea was to treat the North Carolina water with natural humic acid, which is a reasonably strong reducing agent.

It was found by a series of experiments and subsequent analysis of dissolved manganese that humic acids greatly enhance the solubility of manganese oxide in seawater. In other experiments they demonstrated that humic and fulvic acids in river water also cause manganese oxide to dissolve. The process was observed to be strongly stimulated by sunlight, and thus it was concluded that the dissolution is the result of a photoactivated process.

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explains a number of questions raised previously about the Big Bang expansion and, in doing, addresses the monopole question.

As described in the *Science* report, Guth's theory uses the concept that quantum Higgs fields can tie themselves into a knot, thus forming a heavy particle with single magnetic polarity—a monopole—having an equal chance to be of north or south character. By contrast, all known magnetic fields are directly or indirectly electromagnetic in origin and have the property that even when broken, the resulting magnetic parts are dipolar.

There are problems with producing the universe by a Big Bang, some of which involve monopoles. If monopoles can be formed, they would have done so in abundance. Monopoles should be as common in the universe as protons. Monopoles predictably would act as sort of 'antigravitation' devices, and they would, if so abundant, at least have wiped out the galactic magnetic field. Further, the mass excess in our universe due to the monopoles would be huge, enough to have caused the universe to have collapsed back to its point of origin—that is, the Big Crunch idea. The Big Crunch has not occurred.

News (cont. on p. 98)

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News (cont. from p. 97)

curred, and monopoles are not too common, although one may have been observed [see *Eos*, February 22, 1983]. Instead, the density of the universe is in critical balance between Big Bang and infinite expansion. The remarkably precise, symmetrical, and uniform nature of essentially all of the properties of the universe seems to defy theoretical logic. To explain all these unusual phenomena, and to do so within GUTs, Guth invoked an "inflationary scenario," an exceedingly rapid expansion stage within the very first part of the Big Bang.

As argued by Guth, "For the real universe to be so close to criticality now, some 10 billion to 20 billion years after the Big Bang, it must have differed from criticality in the earliest instants by less than one part in 10⁴⁰. An initial condition cries out for an explanation." The explanation fell out of a sort of quantum phase diagram approach. In a particle physics analogy to a crystallization sequence, the whole universe undergoes a series of phase transitions. The Higgs field "freezes out" in this model and in a way firms a field of lattice points that affect quantum particles and their mutual interactions. The way the Higgs field freezes in those models is rather important, the results producing a range of possible structures from a sponge-like character to that of a coagulated mass of jelly. The "jelly" model yields the best results. In its scenario, the universe begins at a point source whose diameter is one millionth that of a proton. The inflation begins, therefore, from a system small enough to have achieved thermal equilibrium. The inflation undergoes a sublimating-out process resulting in a uniform universe. The uniqueness, or unis of them, were lost in this phase transition stage that changed matter entangled elementary particles such as quarks, leptons, and neutrinos into composites such as protons, neutrons, or mesons. Electrons and neutrinos stayed in the free state as they are observed now.—PMB

Study Finds Carbon Mobility in Olivine

The significance of carbon's existence as a solid solution in olivine is many fold. Currently there are speculations about the contributions of dissolved carbon to the electrical conductivity of minerals that constitute the earth's upper mantle, and for this contribution olivine is a prime candidate. If carbon, once dissolved, is relatively mobile in upper mantle mineral structures, it can speculate about diffusional processes in the upper mantle, the very diffusional processes many geophysicists ordinarily assume to be relatively unimportant. Diffusion—creep processes that support convection overturn in the solid-state—can be accelerated, perhaps, by the mobility of minor elements.

In their recent study of carbon in natural olivine, G. Oberhauer, H. Katherin, G. Demontier, H. Gorska, and F. Freund of the University of Köln, West Germany, found that the carbon in olivine exists in a truly dissolved state, not as carbon dioxide related anionic groups or as a graphite-like polymer (*Gesell. Cosmoch. Acta*, in press, 1983). They found that the dissolved carbon has a relatively high diffusion rate which is, needless to say, unexpected for an olivine host. Their study showed that the carbon was not associ-

ated with major lattice defects such as dislocations or subgrain boundaries. The carbon apparently is bonded with the olivine with an approximate C-O⁻ character.

Oberhauer et al. used two specialized methods to study the carbon. They analyzed the carbon by means of observing the nuclear reaction ¹⁴C(d, p) ¹⁴N, and also by the X-ray induced photoelectron spectroscopy (XPS) technique. Carbon analysis by nuclear reaction is a rather novel approach, described by Oberhauer et al. as being a "powerful method and certainly of interest to many petrologists." The beam of a particle accelerator, deuterium ions in this case, accelerated to an energy lying below the Coulomb barrier, is impinged on the carbon. The resulting converted ¹⁴C nuclei to ¹⁴N and in so doing lose energy as a function of depth in a process that can yield depth concentration values based on standardization. Due to interferences from other electron reactions with light elements, the useful depth range of analysis of carbon in olivine is limited to about 2.5 nm. To observe kinetic, diffusional parameters, the changes of the measured profile were observed from low temperatures (liquid nitrogen) to high temperatures (1050 K).

The XPS (or ESCA) technique used in the olivine study was more or less standard. An X-ray beam of Mg₂₄ radiation was made incident onto a specially prepared olivine surface under vacuum. The photoelectrons (Cs, Mg_{2s}, Si_{2p}, and O_{1s}) were detected, but because the depth of penetration was only a few Angstroms, it was necessary to sputter away layer after layer to obtain the carbon concentration profiles. An Ar⁺ ion sputtering device was included in the XPS apparatus.

Total carbon was measured in the olivine crystals by means of an ASTM standard-type carbon analyzer to be on the order of 180 ppm in one specimen and 80 ppm in another. The heating/cooling cycles produced rather sharp profiles of carbon as analyzed by the nuclear reaction and the XPS methods. That the high mobility of carbon observed was due to a thermally activated diffusional process was demonstrated in part by the reversibility. The highly mobile carbon species in these olivines could be made to diffuse to the surface, and then back into the crystal interiors. The driving force for this process was thought to originate in localized lattice strains of carbon atoms in the olivine crystal matrix. Carbon–oxygen bonds would be polar. The carbon species would be driven directionally toward elastically relaxed volumes. The diffusion equation was evaluated by substituting the measured values obtained by both techniques as follows. The equation is given by

$$D_{\text{carbon}}(\text{olivine}) = D_0 \exp(-E/RT)$$

where D_0 , E , and R are the frequency factor, the activation energy, and the gas constant, respectively. From the nuclear reaction data obtained at 7 K:

$$D = 10^{-12} \exp(-7.8/RT)$$

and from the XPS data obtained 450 K < T < 925 K:

$$D = 10^{-14} \exp(-6/RT)$$

(both in m² s⁻¹ and kJ/mole). The activation energies are unusually low for the case of carbon atoms diffusing through a relatively dense packed crystal structure such as olivine. It would appear that if correct, the data imply a great mobility of carbon in olivine.—PMB

The top quartile, the most creative, seemed to be occupied by Ph.D. holders of average age 48 and the bottom quartile, the least creative, by non-Ph.D. scientists of average age 58. The Ph.D. and age factors, if taken separately, show no such correlation, however. Not only does measured creativity have no correlation with age but, as Lyon puts it, "Creativity, as measured by the test, is not a respecter of academic degree."

The way of testing at ORNL appears to be useful in evaluating the creativity of individuals within groups having very high or very low creativity. The test did not, however, examine factors that may enhance the creativity of a presumably creative group of people such as scientists.—PMB

Geophysical Monograph 24

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Can Tests Identify Creative People?

Forum

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It is always a popular pursuit by administrators to assess the creativity or innovative qualities of scientists in order to evaluate their research capabilities. Of course, traditionally such evaluations have been fraught with subjectivity (i.e., innovative scientists are commonly thought to be weird, under 40 years old, independent, risk-taking, etc.), and such evaluations have not been highly valued.

In recent years, through the American Chemical Society (ACS) has attempted to give respectability to the art of profiling the creative of a scientist. ACS, which draws its members from both industrial and academic laboratories, held a symposium on the subject of evaluating the creativity of scientists. The proceedings were published by ACS as "Innovation and U.S. Research: Problems and Recommendations" (W. N. Smith and C. E. Larson, eds., 1980). In the proceedings, as reported in the July 1982 *Chemec* (all quotes here are from the *Chemec* article), A. Nixon was able to identify only the following two partial characteristic of an innovative person:

(1) a low threshold to a state of discomfort with some aspect of the order of things, the status quo; and (2) an extraordinarily high level of mental stimulus enabling him or her to persist until the state of discomfort is removed.

W. S. Lyon of Oak Ridge National Laboratory (ORNL) has evaluated the results of new tests and concluded that, "a simple, reliable measure of creativity seems to be simply to ask the person." He qualifies this by pointing out that, "Such an interrogation must probably be causally put among other questions, perhaps in written form, so that the respondent does not feel obligated to answer face by face." The section on magnetospheric physics, both terrestrial and planetary is quite out-of-

date, but out of place in this report. The philosophy expressed in chapter 7 ("Role of Esimun in Hydrology") by Piering and Kuczera is remarkable, but, most ironically, the authors fall immediately, in their very own chapter, into the pitfalls against which their warning (see the "model building" on p. 91 and the "Instructive" (Fig. 7.2). Chapters 9 (Leopold) and 10 (Baker) on hydrology and geomorphology go together. They are to the point and refreshing. What better way is there to denounce the fruitless efforts of developing better plating position formulae (as an example of little research) than to take a look at Figure 10.2. Chapter 11 (no prediction is a surprise. After all, there is no harm, indeed some noblesse, in conceding (even if only implicitly) that one may have been wrong in the past. Only outstanding scientists can afford such turn around and lead again in a new direction.

At the Ph.D. level, and maybe at the M.S. level as well, the report should be required reading for all graduate students (and professors) specializing in hydrology. In a course on Physical Hydrology, as an antidote to brainwashing, Chapters 7 and 11 should be required reading, whereas for Stochastic Hydrology and/or Conceptual Hydrology, the reading list would include the chapters 1, 3, 8, and 10.

It is a worthwhile report. Read it.

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The Mineralogy, Chemistry, and Physics of Tropical Soils With Variable Charge Clays

G. Uehara and G. Gillman, *Transactions, American Geophysical Union*

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geophysical monograph 25

Physics of Auroral Arc Formation

S.-I. Akasofu and J.R. Kan, editors

The polar aurora is investigated and a unified physical model has begun to emerge based on a great variety of observations and plasma studies. Topics explored are:

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on aquatic ecosystems by Carris) is interesting.

The main emphasis of the book is placed

on the chemistry of constant potential surfaces. Chapter 3, entitled "Chemistry, com-

Books

Cosmic Electrodynamics

J. H. Piddington, 2nd ed., R. E. Krieger Publ., Malabar, Fla., xii + 361 pp., 1982, \$27.50.

Reviewed by C. T. Russell

This highly readable monograph is an extensive revision of an earlier book published in 1969 by John Wiley. Piddington loves his subject matter and writes with enthusiasm about the role and critical importance of magnetic fields in the universe around us. He treats solar magnetic fields, the interplanetary medium, the magnetosphere, planetary magnetospheres, and galactic magnetic fields. The treatment is far from uniform, and the topical balance has shifted somewhat from that in the first edition to that in the second. For example, in the first edition there were 27 pages devoted to solar magnetism, and in the second 81 pages. In the first edition there were three chapters devoted to geomagnetism and in the new edition two.

Piddington takes the reader on a whirlwind tour of the cosmos in a more qualitative fashion than quantitative. The author appears to have wanted to document his understanding of the subject and the reasons for his point of view rather than to write a textbook. However, with a moderate amount of auxiliary material this book could serve as the focus of a course. Alternate hypotheses are mentioned throughout.

Nevertheless, there is much to recommend this book. It is well written and treats a very broad subject area, often with keen insight. It is recommended reading, especially for those interested in solar magnetism and Piddington's rather unorthodox views.

C. T. Russell is with the Institute of Geophysics and Planetary Physics, University of California, Los Angeles, Calif.

Scientific Basis of Water Resource Management

Geophysics Study Committee, *Stud. in Geoph.*; National Academy Press, Washington, D.C., xii + 127 pp., 1982.

Reviewed by H. J. Morel-Seyoum

The least that one can say about the report is that it is very enjoyable reading. Every chapter has been carefully written, and the literary merit of some chapters is outstanding (particularly those by Kleme, "Empirical and Causal Models in Hydrology," and by Baker, "Geology, Determinism, and Risk Assessment"). The best that one can say about the report is that it does meet its stated objectives (1) evaluation of the adequacy of present hydrologic knowledge and of the appropriateness of present research programs to provide information for decision making and (2) description of the impact of hydrologic knowledge on the planning and management of water resources. The worst that one can say about the report is that it is not particularly original and that there are few really fresh new arguments developed in it. One notable exception is provided in Chapter 11, by Matthes, Landwehr, and Wolman, which challenges the traditional implicit assumption that "human activity is an external perturbation of the hydrologic cycle." Though not the explicit intent of chapter 4, by Bredehoef, Papadopoulos, and Cooper, with the explosion of the water-budget myth in groundwater, this chapter illustrates clearly the profound interaction of man (through wells) in the hydrologic cycle, a situation that cannot be comprehended from a study of the system free from human influence.

There is a consensus in the report that there is currently much misguided research. Much hydrologic research is directed at problems that are not necessarily the most significant ones in theory, on the ground, or in practice (Leopold, p. 107), or are "scientificly sterile" (Kleme, p. 99). "Mathematical convenience is a popular excuse, current fashion running a close second" (Kleme, p. 99). Practically, all authors call for a return to a search for basic understanding of the dynamic mechanisms governing the processes (theory), a return to experimentation (not to be confused with the "series collection" of data that sometimes passes for field hydrology (p. 28)) and the abandonment of computer-induced model building, conceptual or statistical, with fruitless evergrowing mathematical sophistication as a research goal in itself. The new focus for research will require "an uncommon degree of cooperation" from a broad range of the earth sciences (Eag

